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# SCIENCE

FRIDAY, JULY 28, 1911

MECHANISMS OF CELL ACTIVITY<sup>1</sup>

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EVERY scientist who concerns himself with the problems of his own specialty must devise for himself certain concrete pictures of the nature of the fundamental units with which his specialty deals, in order that he may have a concrete form in which to clothe his thoughts. Thus each chemist must form for himself some sort of concrete notion concerning fundamentals, like atoms, molecules, chemical affinity, valence and ionization, imagery which he must avoid mistaking for absolute reality, and which he must be ever ready to shift and change and modify, in accordance with the development of chemistry.

The biochemist also has his imagery, only he takes the data of the chemist and physicist as the material out of which he constructs an imagery of his own, dealing not with atoms or molecules as such, but with conceptions of the physical and chemical nature of protoplasm.

I would present to you to-day the hypothesis which some biochemists have developed for themselves concerning the structure of protoplasm and the cell. Such a presentation must be very largely a personal one, for two biochemists would hardly be likely to agree on all the details, however much they might be in accord on the essentials. Consequently, what I am about to offer will contain nothing essentially new.<sup>2</sup>

<sup>1</sup> Address presented before the general meeting of the American Chemical Society at Minneapolis, Minn., December 28, 1910. Published by permission of the Secretary of Agriculture.

<sup>2</sup> For an earlier presentation cf. Hofmeister, Fr., "Die Chemische Organisation der Zelle," Braunschweig, 1901.

My excuse for offering it at all is that it may present views novel to an audience such as this; views that may be of interest because they deal with some of the phenomena of life.

The fundamental unit of the biologist was for many years the cell. The conception of the cell is really an anatomical one, and a mere anatomical consideration of it does not lead to an understanding of its functions. I can illustrate what I mean by discussing the ordinary gravity cell or battery. The anatomist would describe such a cell as consisting of a vessel of some sort of material containing a blue liquid in which there were a whitish mass and a reddish mass at different levels, each connected with a reddish strand projecting upward. Such an analysis is obviously imperfect because it would never lead us to discover that the cell is capable of producing an electric current. However, he who studies the current produced by such a cell, its external and internal resistance, and the like, is, in respect to the gravity cell, a physiologist. Such a physiologist gives us some insight into the functioning of the cell, but he tells us nothing concerning the cause of the generation of the current. The analytical chemist, on the other hand, would tell us that the gravity cell consists of such and such a percentage of silicates, zinc, copper, sulphuric acid and moisture, information very useful in its way, but no more so in the comprehension of cell activity than that furnished by the anatomist and the physiologist. To understand the gravity cell all three kinds of knowledge are essential. Structure counts quite as much as chemical composition. In such instances the chemist needs anatomical knowledge, for with chemical reagents alone one can not recognize structure. Without a knowledge of structure it is often quite as impossible to understand mechan-

isms as it would be to predict the regular movements of a watch by first smashing it and then determining by analysis that it contains certain percentages of gold, copper, carbon and tin *et al.*<sup>3</sup> Watches may be made of many kinds of material and yet keep the same time. However important the material, it is the structure that is even more essential in measuring time. Apparently this seems equally true of living organisms. The energy moving a watch may be furnished by a spring of brass, rather than of steel. Similarly most living things obtain their energy by the oxidation of carbohydrate, but by no means all. For example, certain kinds of microorganisms, the Beggiatoaceæ, obtain their energy by the oxidation of sulphuretted hydrogen to sulphuric acid. They absorb sulphuretted hydrogen and excrete sulphuric acid. Most living things store up, as reserve sources of energy, some form of carbohydrate, such as starch or glycogen. Not so the Beggiatoaceæ; they store up elemental sulphur. The conclusion is therefore inevitable that for a proper understanding of the mechanisms of the living cell both structural and chemical knowledge is necessary.

Evidently the earlier notions concerning protoplasm were unsatisfactory because they were either purely anatomical or purely chemical, according to the bias of the investigator. Originally protoplasm was regarded as a material showing some anatomical structure, to be sure, but chemically more or less homogeneous, though very complex. The very term "protoplasm," the first formed, is nothing more than a definition of this conception. Later, as biochemistry advanced, protoplasm came

<sup>3</sup> Similar opinions have been expressed, among others, by E. H. Starling ("The Mercers' Company Lecture on the Fluids of the Body," London, 1909).

to be regarded as a very complex though rather homogeneous mixture of materials, some of which were assumed to be alive. The latter were supposed to be huge, complex molecules, protein in the main, but not necessarily entirely so. The vital qualities inherent in protoplasm were supposed to run parallel with the complexity and instability of these huge living molecules. Essentially this was a chemical conception of protoplasm different from ordinary chemical conceptions only in that these hypothetical, hopelessly complex molecules were assumed in some way to possess the power of regenerating themselves when in consequence of their instability they fell to pieces. As we look back now, we can see that apparently the only reason why proteins were chosen to fill this rôle was that at that time far less was known about proteins than about the other constituents of cells. By some it was even imagined that if only it were possible for organic chemists to know the structure of such molecules completely, a deep insight would be gained into the causes of life. Others, less sanguine, believed that this could never be, because in the process of analysis the molecule ceased to live, was changed, depolymerized, so that what was studied had in the very process of study lost its vital properties. Even as recently as the beginning of Emil Fischer's work upon protein the hope was expressed that since he had brought the synthesis of protein within striking distance, a more intimate knowledge of the nature of life would follow. And yet, though it is now possible to make substances very similar to some proteins with very high molecular weights, and though we may rest assured that all the complex substances occurring in living things will eventually be so well known that they will be ranged without wonder and without comment with the vast horde

of organic compounds, nevertheless, by such achievements alone, only a limited insight into the mechanisms of life can be gained. The reason is that a study merely of chemical constitution, however necessary, will carry us but a very little way in understanding even the simplest processes which take place in protoplasm, unless it be combined with a study of structure, and of the dynamics resulting from both.

Now when I speak of structure I do not mean necessarily anatomical structure that is visible with the microscope. Still a study of even microscopic structure has been and is of incalculable value. With such studies, the name of Bütschli is indissolubly connected. He it was who first emphasized the fact that protoplasm has the structure of a foam or a network with interstices filled with material of a physical nature different from the network, or finally, the structure of an emulsion. Indeed, his anatomical observations led him to study foams, and emulsions, experimentally, in the hope of being able to interpret his anatomical studies more rationally. His experiments became of fundamental importance in stimulating work in certain fields of pure colloidal chemistry and of the dynamics of surfaces. He understood better than any one before his time the anatomical structure of protoplasm. He was the first to point out that protoplasm is heterogeneous, consisting of at least two phases touching each other by minimal or capillary surfaces.<sup>4</sup> We shall learn to appreciate the great significance of this idea. Even Bütschli, however, found that many heterogeneous systems appeared homogeneous under the highest powers of the microscope, because the differences in refraction between the phases was insufficient to render them visi-

<sup>4</sup>O. Bütschli, "Untersuchungen ueber mikroskopische Schäume und das Protoplasma," Leipzig, 1892.

ble.<sup>5</sup> Visibility depends upon size, differences in refraction, adsorption of stains or reagents and similar more or less accidental phenomena. Plainly there may be physical and chemical structure which does not happen to be visible. It is to such structure dependent upon the heterogeneous nature of protoplasm and to the consequences arising from its nature that I wish to draw your attention to-day.

Protoplasm must have some such organization, other than a merely chemical one. Otherwise we can not understand how so many intricate reactions can take place in an orderly fashion within the narrow confines of a single microscopic cell. Let me illustrate by an example. The yeast cell converts sugar into alcohol, carbonic acid and water. Under certain conditions it also converts sugar into glycogen which it may store for a long time within itself, or which it may soon reconvert into sugar and then into alcohol. Under certain conditions it may oxidize alcohol. It synthesizes protein and cellulose. It forms glycerine, succinic acid and amyl alcohol. It may reduce sulphur to sulphuretted hydrogen. It performs undoubtedly a whole series of cleavages, syntheses, oxidations and reductions, and yet, examined under the microscope, it appears fairly homogeneous. No structure is visible capable of explaining how in this small space so many processes can go on side by side in an orderly fashion without interfering with one another. In a single test-tube it is manifestly impossible. In any homogeneous medium it is manifestly impossible. However, in a heterogeneous medium, such as an emulsion, it is conceivable. We have merely to imagine the reactions as taking place in different phases and to remember that at the points of contact of two phases membranes

form. By the term "membrane" we must understand in this connection merely the condensation of material at a surface serving to separate two phases. We have then imagined a structure for protoplasm, part chemical, part physical, sometimes visible, sometimes invisible, in which many reactions might go on side by side as thoroughly separated as though in separate test-tubes. They would, however, go on more effectively than in separate test-tubes, because in these no influence of one reaction upon the other is possible, whereas in a heterogeneous system such an influence between two separated reactions is conceivable. A reaction taking place in one phase, may, either by yielding products soluble in another phase, or by changing concentrations, affect one or two or every other phase. Interrelations of this kind might very well lead to what is ordinarily termed coordination.

Indeed, there is some evidence that some of the coordination is of this type. When a piece of protoplasm dies, not all the functions which up to that time it has exercised, cease at once. Some of them may continue for a long time, and these are usually due to enzyme action. In the dying mass respiration may continue as well as many other functions, but while they may be qualitatively the same, quantitatively they differ. The balance present during life is destroyed and certain reactions gain the upper hand, eventually dominating the field till everything else is suppressed. Coordination, the great characteristic of life, disappears. Anything which intermixes protoplasm, hence disturbs the phases, destroys coordination. Freezing is one of the agents exerting this effect upon many forms of protoplasm, for by causing some of the water to crystallize, it most effectively disturbs the balance of the phases. We note this, for instance, in the

<sup>5</sup> Ostwald, Wo., "Grundriss der Kolloidchemie," S. 32, Dresden, 1909.

freezing of potatoes. Potatoes contain a mechanism for converting starch into sugar. Ordinarily this mechanism is controlled by and coordinated with other mechanisms. Freezing disturbs this balance. The mechanism for the hydrolysis of starch runs riot and the potato becomes sweet. Another instance is the coloration of the leaves in the autumn. Leaves contain among other things chlorophyll, chromogens and an oxidation mechanism. Under ordinary conditions this mechanism is held in check. Frost, however, plasmolyzes the cells, intermixes the protoplasm, and the oxidations run riot. Chlorophyll is bleached so that yellow pigments, such as carotin and xanthophyll, which are ordinarily masked by the green, become evident, while other substances, either pre-existent or formed during plasmolysis, are converted into the brilliant pigments of our autumn woods.

Now ever since the processes of life have been studied at all chemically, the ease and efficiency with which protoplasm brings about reactions has filled the chemist with awe. Reactions which he is able to perform *in vitro* only with difficulty by the use of powerful agents and high temperatures, protoplasm brings about perfectly at low temperatures. If we pursue our conception of protoplasm as a heterogeneous system further we can imagine mechanisms by which such action is conceivable.

Many reactions go on rapidly in protoplasm to a high degree of completeness, while *in vitro* the rate may be slow and the yield insignificant. This becomes intelligible in a heterogeneous medium without necessarily resorting each time to the action of an enzyme. We have only to assume that the reaction takes place in one phase and that one or more of the products disappear into another phase as fast as

formed. In this way reactions might go on at a rate and to an equilibrium quite different from those *in vitro*, for the products of the reaction would be removed from the seat of reaction almost as fast as formed. Such a mechanism might be merely the result of different solubility in the phases.

A similar line of thought throws light upon the fact that a substance present in protoplasm as a whole, in such minute concentration that *a priori* its influence ought to be negligible, may nevertheless exert a profound effect. We need not necessarily assume that this apparently disproportionate effect is produced by a catalytic mechanism. In a heterogeneous system the concentration of a given constituent as determined by quantitative analysis of the whole may be a spurious value, because this constituent may not be evenly distributed throughout the heterogeneous system. It may be almost absent in one phase and concentrated nearly entirely in another, and this difference in distribution may also be merely the result of difference in solubilities. Hence in protoplasm substances may be concentrated greatly in definite localities and thus make reactions possible which at low concentrations would be infinitely slow.

An uneven distribution of the various constituents of protoplasm such as this, is not the only way in which the concentration may vary according to this multiple phase hypothesis of protoplasmic structure. Even in the same phase the concentration of the substances in that phase may be caused to vary greatly by the energy at work in surfaces. It is a well-known fact that substances which diminish surface tension or the tendency inherent in liquids to assume that shape which reduces the surface to a minimum, accumulate at the surface so that they are present in the thin

surface layer in greater concentration than in the interior. Now in such a system as we have imagined protoplasm to be, the sum of the areas of the surfaces of all the phases must be very great, and consequently the concentration of the various substances distributed through it must vary greatly, not merely according as they are distributed in one or the other phase, but also as they are concentrated still more at the surfaces of one or the other of the phases. This concentration at surfaces may be very great so that the substances are present as though under great pressure, and we must imagine that reactions are facilitated at surfaces just as reactions with gases are facilitated by high pressures. Finally, we can imagine reactions facilitated whenever conditions arise which diminish surface energy, for in that case free energy is produced in the narrow concentrated surface film. How this might facilitate reactions we can only conjecture.

So far we have considered only the grosser phases. Finer phases, however, exist in protoplasm. It is now pretty well established that colloidal solutions are multiple-phase systems. Cells contain colloids in solution, and these colloids usually are of the type known as emulsion colloids, which means that all the phases of the colloidal solution are fluid. We have then here phases of very minute dimension. Now it is well known that the energy acting at surfaces increases relatively with the increase in curvature of the surface. The curvature of these minute colloidal phases is very great, and the energy which acts in these infinitely curved surfaces is correspondingly powerful. It is quite possible that many of the phenomena which are termed enzymotic are brought about by forces of this type, for it is very probable that the class of agents termed enzymes is not a uniform one, but includes

many classes of quite different agents acting by as many different mechanisms.

We have seen how considerations of structure have led to conceptions of cell dynamics. Conversely, a consideration of these dynamics can lead us back to a deeper understanding of structure.

Protoplasm ordinarily contains 80 per cent., and upwards, of water. Some beings may contain even more. For example, the medusæ or jelly-fish, fairly firm structures though they are, contain but 3.7–4.6 per cent. of solids.<sup>6</sup> Of these solids over 3 per cent. are the sea salts, so that the bell of the medusa, as solid as a firm jelly, can almost be said to consist of organized seawater. Ordinary protoplasm is not as thin as this. Still of its 15 or 20 per cent. of solids a considerable portion is inorganic salts and other electrolytes, for the greater part in solution, so that they hardly produce solidity. The remaining substances consist of fats, proteins, lipoids and other colloidal material. Built of such materials, it is hard to see how an organism can have so nearly solid or rather semi-solid a structure as protoplasm. If, however, we imagine the materials of which protoplasm is composed as distributed in different phases, the difficulties are not so great. If we imagine the fat and the lipoid as present in a different phase from the water, being present as an emulsion, perhaps rendered permanent by some such substance as soap, and if we think of other substances such as the proteins present in a colloidal and viscous state, and, if we imagine both the crystalloids and the colloids distributed between the various phases, we can get a structure which will be as firm as protoplasm is known to be. Thus it is easy to take egg-albumen, oil and sugar solution and mix them so thoroughly that the re-

<sup>6</sup> Krukenberg, "Ueber den Wassergehalt der Medusen," *Zool. Anzeiger*, 1880, S. 306.

sulting emulsion is firm enough to handle with a shovel. A firm system of this sort is used technically as a lubricant. By the emulsification of certain heavy oils with less than one per cent. of water an emulsion so solid may be formed from the two liquids that it may be whittled with a knife.<sup>7</sup> These considerations, however, do not tell the whole story, at any rate for plants. A part of the firmness of many plant structures is due to the phenomenon termed by plant physiologists "turgor." This phenomenon has much similarity to the inflation of a flabby, hollow, elastic balloon with gas. In turgor, however, the inflation is by water, not gas, and the inflating force is not mechanical but osmotic. How far turgor is responsible for rigidity in animal structures is not yet clear. Certainly something very similar exists in the red blood corpuscles.

We have been considering the concentration of substances upon surfaces of finely subdivided phases; but in obedience to the same laws concentration takes place upon larger surfaces. This phenomenon must occur not merely at the contact surfaces between the phases in the interior, but also upon the outer surface of the protoplasmic mass itself. In such places we must have a concentration of material. Indeed, it can be shown experimentally that many solutions form really quite firm membranes even when there is no chance for evaporation to take place.<sup>8</sup> Very probably

<sup>7</sup> Ostwald, Wo., *op. cit.*, p. 105.

<sup>8</sup> Ramsden, W., "Abscheidung fester Körper in den Oberflächenschichten von Lösungen und 'Suspensionen'" (Beobachtungen über Oberflächenhäutchen, Emulsionen und mechanische Koagulation), *Zeitschrift für physikalische Chemie*, Bd. 47, S. 336.

Metcalf, M. V., "Ueber feste Peptonhäutchen auf einer Wasseroberfläche und die Ursache ihrer Entstehung," *ibid.*, Bd. 52, S. 1.

Zangger, H., "Die Immunitäts-Reaktionen als physikalisches spez. als Colloid-Phänomen," *Vier-*

this phenomenon is responsible in many instances for the formation of biological membranes and may also account for the differentiation of the external layer so frequently seen in cells. This might merely be the result of the concentration of material in the outside layer in consequence of surface action.<sup>9</sup> Considerable evidence for the participation of surface forces in cell-membrane formation may be found in the studies on hemolysis, by which is meant the leaking of hæmoglobin through the membranes of blood corpuscles. Many of the substances which cause the cell membranes of the red blood corpuscles to lose their semi-permeability in this way, have a great influence on surface tension. Such are soaps and saponine. One of the actions of certain snake venoms is dependent upon the presence in the venom of a substance of this type.<sup>10</sup>

If this hypothesis of concentration of material at the cell surface be correct, then it is easy to understand how many cells have the power of regenerating a new membrane on a wound surface, such as is formed when an amœba is cut in two. The surface energies must begin at once to act at the new surface until it, too, has been brought into equilibrium with the interior just like the rest of the cell surface.<sup>11</sup>

This hypothesis of membrane formation can not be applied, for the present, at any rate, to many specialized membranes such

*teljahreschrift der Naturforschenden Gesellschaft in Zürich*, Jahrgang 35, S. 441.

<sup>9</sup> Overton, E., "Ueber den Mechanismus der Resorption und der Sekretion in W. Nagel's 'Handbuch der Physiologie des Menschen,'" Band II., Teil II., Seite 805-6.

Pfeffer, W., "Osmotische Untersuchungen, Studien zur Zellmechanik," S. 124, Leipzig, 1877.

<sup>10</sup> Zangger, H., "Ueber Membranen und Membranfunktionen," *Ergebnisse der Physiologie*, Bd. 7, S. 138, 1908.

<sup>11</sup> Cf. Overton, *op. cit.*; Pfeffer, *op. cit.*, and Zangger, *op. cit.*



as are found in many plant cells and in the red blood corpuscles. Still surface forces may be concerned even in these morphologically distinct and permanent structures. Now colloidal substances are among those which tend to accumulate at surfaces. Many of them are easily coagulated, and changed in such a way that they become more or less permanently insoluble. Such colloids are said to be irreversible, because after having been put out of solution, they can not easily be made to revert into solution again. Irreversible colloids when they become concentrated at a surface tend to become coagulated. In this way membranes of a high degree of tensile strength and permanency may be made experimentally.<sup>12</sup> Something of the sort might very well occur in cells with a morphologically distinct cell wall.

Of the composition of cell membranes we have in the last decade achieved certain definite notions. The cell membrane is usually semi-permeable, only allowing certain substances to penetrate into the cell. Now there are a number of ways in which the semi-permeability of a membrane may be explained. One is that the penetrating substance dissolves in the membrane. Another is that it combines loosely with it. So if we note what substances penetrate into cells and what the solubilities of these substances are, we may be able to reach certain conclusions concerning the nature of the cell-membrane. This has been done and it has been shown that many of the substances which enter cells are very much more readily soluble in fats and lipoids than in water. Indeed, the narcotic effect upon cells of many indifferent substances is proportionate to their partition coefficient between water and oil. For apparently the same reasons free alkaloids which are soluble in oil seem to penetrate cells,

<sup>12</sup> Metcalf, M. V., *op. cit.*

whereas their salts only do so in so far as they are dissociated. For similar reasons undoubtedly the toxicity of certain metallic salts, such as the chlorides of copper and of mercury, is due in part to the fact that, being soluble in organic solvents, they enter cells rapidly.

The objection has been raised to the hypothesis of the lipoid nature of the cell wall that it does not explain how certain substances like sugar, protein and inorganic salts which are all undoubtedly utilized by the cell in one form or another enter the cell. It has, therefore, been suggested that the cell wall has a mosaic structure, other material besides lipoids entering into its composition.<sup>13</sup> This is very probably true, for if we are right in assuming that forces acting at surfaces take part in the formation of the cell wall, then all those substances which are present in the cell, and which have the property of diminishing the surface tension of water, will affect one another in regard to their concentration at surfaces. It is the same phenomenon that has been so much studied in the influence of one substance upon another in respect to adsorption upon solid surfaces.<sup>14</sup> How different substances may influence the concentration of one another on surfaces such as those of cells we can as yet only conjecture, but it is entirely possible that the result may be a mosaic structure of the membrane. If this suggestion prove true, it is possible that protein takes part in the structure. It may be then responsible for the entrance in small amounts into cells of certain substances as required by the metabolism. Proteins, in their capacity as amphoteric electrolytes

<sup>13</sup> Cf. Macallum, A. B., "Cellular Osmosis and Heredity," *Transactions of the Royal Society of Canada*, 3d Ser., Vol. 2, pp. 152 et seq., 1908.

<sup>14</sup> Michaelis, L., "Dynamik der Oberflächen," S. 25, Dresden, 1909.

combine with salts.<sup>15</sup> Indeed it is virtually impossible to prepare protein free from ash.<sup>16</sup> It may be that salts enter protoplasm by combining with protein in the membrane. Even if this mechanism prove ultimately not to exist, all the possibilities are not exhausted. The lipoids, kephalin and lecithin, occur in combination with potassium and sodium.<sup>17</sup> These compounds are freely soluble in anhydrous ether. The metal is not completely masked, but can become to a slight degree dissociated. Perhaps it is by forming such compounds that metals enter cells.

I hope I have shown that by the methods of the organic chemist alone we can not hope to achieve much insight into the mechanisms of protoplasm. These mechanisms are dependent upon structure and this organic chemistry is not capable of revealing. The mechanisms are themselves interrelated and coordinated. These relations and coordinations are not capable of study by the usual analytical methods. The process of analysis destroys them as it destroys life itself of which they are the most characteristic manifestations. These characteristics of life can be approached only from the basis of structure of some sort. For a proper understanding of it, anatomical, chemical and physical knowledge must be combined. The resultant alone offers the hope of widening our knowledge of the mechanisms of cell activity.

CARL L. ALSBERG

<sup>15</sup> Cf. Mathews, A. P., "A Contribution to the General Principles of the Pharmacodynamics of Salts and Drugs," "Biological Studies of the Pupils of W. T. Sedgwick," pp. 103-4, Boston, 1906.

<sup>16</sup> Harnack's ashless protein is really a protein with volatile ash—HCl. *Berichte der deutschen Chemischen Gesellschaft*, Bd. 23, S. 3745, 1890.

<sup>17</sup> Koch, W., and Pike, F. H., "The Relation of the Phosphatids to the Sodium and Potassium of the Neuron," *The Journal of Pharmacology and Experimental Therapeutics*, Vol. 2, p. 245.

# THE TOTAL SOLAR ECLIPSE OF APRIL 28, 1911

[PRELIMINARY COMMUNICATION]

ON the way to meet the *Carnegie* at Colombo, Ceylon, I was so fortunate as to make immediate connection at Suva, Fiji, for Apia, Samoa, by means of a small steamer, the *Dorrigo*, chartered by the German government to carry the mail. I journeyed next to Pago Pago, Tutuila, 80 miles distant from Apia, chartering a 30-ton motor boat and arriving at Pago Pago on Monday, April 24. Laying my plans before his excellency, the governor of Tutuila, Samoa, he very courteously put at my disposal the U. S. cruiser, the *Annapolis*, and furthermore gave me the assistance of some of his best officers and men.

When I left Washington on March 16 the possibility of getting into the belt of totality in time seemed too small to warrant taking with me skilled assistants or elaborate outfits for chance eclipse observations. However, I took two magnetometers and Mr. Abbott, of the Smithsonian Institution, kindly provided an improvised hand-driven, double-lens camera of about 11½-foot focus; everything was packed in water-tight cases so as to be prepared for difficult landings. I decided, namely, to get, if possible, on one of the islands not occupied by any eclipse party which, while equally desirable, were not as accessible as the Tongas where all the parties congregated.

The *Annapolis* left Pago Pago, Tuesday night, April 25, and arrived at Tau Island—the nearest accessible island in the belt—the following afternoon. The entire outfit was landed without mishap through the breakers on the northwest side of the island, near the village of Tau; this part of the work was entrusted to Capt. Steffany, a well-known pilot in these waters. By the time the instruments were unpacked and assembled and suitable sites chosen, night came on. We were comfortably quartered in Vaitupu's house, the widow of Tuimanua, who died a couple of years ago and who, during his time, was the most powerful king of the Apanua group.